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Interactive comment on "An attempt at estimating Paris area CO_2 emissions from atmospheric concentration measurements" *by* F. M. Bréon et al.

Anonymous Referee #2

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Review of "An attempt at estimating Paris area CO2 emissions from atmospheric concentration measurements"

Overview: Beron et al.'s manuscript is focused on attempting to use atmospheric observations of CO2 in the Paris region to constraint CO2 emissions. They present measurements collected at 5 sites in and around Paris, and perform two different flux inversions—one using all five sites and inverting for fluxes, the second approach inverting the gradient between sites focused on three locations. ECMWF winds are used to drive the CHIMERE model, and a linear Bayesian approach is taken to optimize prior inventoried emissions. The authors report optimized fluxes for two 30-day periods, with key findings that the Eiffel tower station to be poorly represented by their framework and the gradient flux method appears greatly superior to inverting absolute concentrations.

Overall this paper is appropriately placed in ACP, is interesting, and much of the analysis and conclusions are sound. As currently presented, the manuscript reads more like a class project than a complete scientific analysis, and improvements on specific aspects of the analysis in addition to organizational and text refinements are needed, after which publication in ACP would be appropriate.

Major Issues: The two larger issues I have are: 1) Presentation/style. This is perhaps a more minor comment, but would require some reworking of the manuscript. The manuscript reads more like a report than a completed analysis/paper. There is some meandering in the language—leading to some repetitive sections and causing some confusion with the reader. A more focused rewrite emphasizing on the specific important elements would be preferred.

2) The Eiffel tower data-model mismatch. One would think this would in fact be the easiest data to simulate, and also would be the most representative and useful in the inversion. The finding that this site seems to be inconsistent with the model is worrisome—and also would imply that the inversion system is not properly representing vertical exchange, therefore biasing the analysis with the surface sites. This disagreement needs to be discussed in more detail and better understood. Is the disagreement greatest at night/during the day? Does it appear to be link to erroneous mixing heights? Is there evidence of persistent eddies developed around the tower causing elevated signals not represented in the model? There is currently very little discussion on this, and we need to see more discussion and data to better understand the potential reasons for failure at this site and possible implications for the inversion system. This likely should be a little section to itself including figures. Depending on why the mismatch occurs, it would impact the conclusions made about the remainder of the inversion system.

2a) The concentration gradient method. It is interesting that this seems to work better, and the message of importance of constraints on incoming CO2 levels is very important—many networks have been designed more recently (see LA) with boundary

values kept in mind. However, the gradient method applied in the inversion here is only one method to constrain this, and a very simplistic one at that. It is important to make it clear that there may be other methods to constrain the incoming CO2 concentration that are not quite as limited in their application.

Minor Issues: Pg. 9649 line 10. Would be appropriate here to add citation for the study preceding the McKain paper (Strong et al., 2011, JGR) and for satellite attempts at the problem (Kort et al., 2012, GRL).

Pg 9649 line 27: Would be appropriate here to cite network footprint/ design studies calculated for Los Angeles, both of current and future observations (Newman et al., 2013 ACPD; Kort et al., 2013, JGR).

Pg. 9651 lines 1-2: It is a simplification that is not necessarily accurate to state the atmospheric transport modeling will be simpler for Paris—the flat topography and winds lead to more of a plume like structure, which may in some senses be simpler to simulate, it necessitates a much tighter requirement on the simulation to get the plume location and dilution very accurate relative to the observations, which may be more different to simulate than a city that accumulates more of a dome due to surrounding topography.

Page 9653 line 3: is this miswritten? Or is the repeatability not know "expected to be better than 0.3 ppm"

Pg. 9653 line 10: Are these local or UTC times? Is there daylight savings?

Pg. 9654 Line 4-5: I am surprised by the day choices—in the US we know Monday and Friday are both distinctly different from mid-week or weekend, and I would expect the same in Europe. I would think the subsection of simple weekday would lead to some bias errors those days.

Section 2.3: Is there any validation at all for the biogenic component? Can you leverage anything from the Lac. et al paper? Are these urban NEE or rural? Is there any city

distinction? We need more description of what is done here, since the simulated NEE is actually important in the inversion result.

Pg 9656 Line 8: The lack of an urban scheme is potentially worrisome. We know from observations that Paris has an elevated pbl relative to its surroundings—is that represented in the current simulation? Is there a justification for no urban scheme inclusion?

Pg. 9658 Line 20-23: This is very important—boundary conditions matter a lot. This in fact is part of the message of this paper.

Pg. 9658 Line 25: This comment doesn't make sense to me—it should matter what the wind direction is—there are very different background concentrations/far upwind sources in different directions of Paris.

Pg. 9659 Line 1: which is bigger by 30 ppm? It is very hard to discern in the current figure. Line 2-4: We need far more quantitative discussion of this, as opposed to just a couple guesses. We need more data presented and understanding for why this discrepancy occurs. Line 12: specify for this flux inversion—not necessarily true of all inversion systems Line 14: this has not really justified this decision—it is more of a qualitative support.

Pg. 9660 Line 10+: This would impact other sites as well? Pg. 9662 Line 10-13: Why this selection for the NEE? One might expect NEE to change fairly drastically day to day given variation in say PAR. Line 21: It would be nice to directly use observations to constrain this.

Pg. 9663 Line 19-27: Worth noting in this portion of the text that the LA network and the updated INFLUX network are designed specifically to account for the upwind boundary conditions.

Pg. 9664 Line 11-22: I understand there are some arbitrary choices made here—but I would like to see somewhere (perhaps a supplement) that explores the impact of

these choicesâĂŤhow a range of selections would impact your findings

Pg. 9665 Line 10: "We deduce"âĂŤHow? Where? You need to tell & show us. Line 5-15: I am a bit worried on this approach as by definition it down weights where observations and model simulations disagreeâĂŤwhich may indeed be due to problems with emissionsâĂŤexactly what you are most interested in!

Pg. 9666 Line 10: Is there any error analysis of the winds? Do we have any information on how accurate winds are? PBL heights? Or any other meteorologically critical variable for these comparisons? This would be important to add and include.

Pg. 9667 Line 20-21: this is also likely due to poor transport representation of accumulation instead of local sources

Is there an analysis of the footprint of the sites? This would seem a critically useful figure. How do we know the representativeness of the sites? The changing daily footprint could better explain agreement/disagreement. This also might be helpful in understanding the limitations of the EIF site.

Pg. 9669 Line 9-10: That is assuming the prior spatial distribution is accurate.

Pg. 9675 Line 14: TypoâĂŤconstrain should be constraint

Pg. 9678 Line 29-30: Specify this statement is just for Paris.

Figure 2: For what time of day is this?

Figure 3: Why is home heat higher on weekdays? This seems totally counter-intuitive.

Figure 6: Please add more labels on the x-axis (here and elsewhere)

Figure 7: The large NEE adjustment is worrisomeâĂŤis this large predicted sink actually realistic?

Figure 9: Please make arrows bigger and clarify this figure that is currently hard to read/interpret. Between what station and what other station? Hard to discern from the

caption.

Figure 12: I see the contrast between this and Figure 7 as indicative the statement that NEE and anthropogenic sources can be spatially distinguished to be erroneous.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 9647, 2014.